GROWTH OF KNOWLEDGE: DUAL INSTITUTIONALIZATION OF DISCIPLINES AND BROKERAGE[1]

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0. Institutionalizing epistemology

Writing more than forty-five years ago, Marlan Blisset put it very clearly (1972, p. 2):

> [P]resent scientific growth is unimaginable without institutional means for coordinating the dynamics of research and directing these activities against increasingly specific problems.

What Blisset gestured towards is a new approach to epistemology. If you want to understand how knowledge grows – how we learn new things or develop more reliable knowledge, and the like – then, in contemporary circumstances, you have to understand the workings of the *institutions* in which knowledge claims are developed and tested.[2]

Since activities aiming at "scientific growth" characteristically unfold, within the wider scholarly community, in a specifically *disciplinary* setting (e.g. philosophy or biochemistry), the institutions that constitute that discipline will be relevant to understanding the development and evaluation of those activities and their products and hence are, on the approach recommended by Blisset, of potential epistemological significance.

Identification of the character and the affordances of disciplinary institutions will therefore constitute one crucial part of the analysis undertaken here.

A second crucial part of the analysis attempts to extend the understanding of "scientific growth" and its facilitators and inhibitors. In particular, where "classical" philosophy of science understood "the dynamics of research" in terms of an "essential tension" (Kuhn, 1977 [1959]) between conservative "normal science" on the one hand and, on the other hand, the challenges posed to normal science by anomalous phenomena or by heuristic degeneration into ad hoc adjustments of guiding (i.e. paradigmatic) ideas and models (see for example Kuhn (1970); Lakatos (1970)), later work has identified another potential impediment to or inhibition of scientific growth, one that only emerges clearly when enquiry is understood in terms of the collective activities of "communities" of researchers, rather than, as Wolfgang Stegmüller put it (1975, p. 94), in terms of a "statement view of theories".

In this paper I will therefore consider how there are strong institutionally-mediated reasons for scholars to shape their activities in ways that may threaten the growth of knowledge, in particular because or to the extent that incentives associated with research activity lead scholars into what Barbara Levitt and James March have called (1988, p. 322) "competency traps". As Daryl Chubin and Terrence Connolly long ago put it (1982, p. 294, emphasis added):

> [T]here exist important pressures which lead to undue persistence of individuals in some research [approaches] rather than others; … the social processes associated with the development of these [approaches] tend toward conservative pressures for intellectual continuity on new entrants; and … the aggregate result of these processes

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> is that far from a wide dispersion of research efforts around the boundary problems of a specialty, *there will be unproductive over-concentration on some few problems, while high-potential areas go underdeveloped.*

Notice that these phenomena of "undue persistence" and "unproductive overconcentration" don't depend on anomaly or heuristic degeneration. These phenomena represent, as suggested, *another kind of way* in which growth of knowledge may be threatened, one which is about the demography of the research community, if you will, and one that can arise even in the absence of anomaly or ad hoccery. (It is notable, and regrettable, that, notwithstanding some attention to a related issue under the heading of Kuhn's "risk-spreading argument",[3] this issue has not been prioritized by mainstream philosophers of science.[4])

I will, therefore, hereafter consider the institutional conditions under which "unproductive overconcentration" can occur, and, crucially, the institutionally embodied opportunities for the avoidance or amelioration of such a threat to "growth of knowledge".

Obviously, it is an empirical matter how often and for how long and in what sorts of research specialties scholars fall into unproductive "competency traps". It is nevertheless clear that, somewhere in the larger institutional ecosytems that superintend "scientific growth", there are counter-vailing incentives or opportunities, since, demonstrably, communities of scholars do not always or inevitably become trapped in this particular way by their commitments to the paradigms that guide their research. In this paper, I will offer an hypothesis about one mechanism that might provide a counter-weight, for individual scholars, to the otherwise conservative incentive structure which they typically face, namely, the academic department as an institutional form and some of the affordances associated with the work-in-progress seminar which is, across a range of disciplines, such a conspicuous feature of scholarly life. While each individual within a research specialty may well face an incentive structure that rewards "normal science" conformity, every such individual will also find, in her departmental colleagues, potentially rich sources of "outside the box" thinking, and so long as some scholars effectively tap these sources, the desired "wide[r] dispersion of research efforts" may well ensue or at least be disinhibited.[5]

I will show, then, how the *dual institutionalization* (Abbott, 2001, p. 126) of the disciplines may sustain a balance of opportunities and incentives, a "dynamics of research" in Blisset's terms, that, when properly "tuned", enables both collective persistence in established practices and exploration of "high-potential areas" outside the mainstream, thus sustaining "scientific growth".

I proceed as follows.

In section 1, I consider the dual institutionalization of disciplines, in terms of departments and research specialties, noting, in the case of the specialties, that these are differently organized in different disciplines and focusing, for present purposes, on the organization of research specialties in particular kinds of disciplines – those described by Tony Becher (1989, pp. 79ff) as "urban" or by Richard Whitley (1984, pp. 193ff) as "polycentric professions". In developing my analysis, I will consider two important "reference groups" for the individual scholar, namely, his departmental *colleagues* and his specialist *peers.* It will transpire, in due course, that colleagues have different contributions to make to a scholar's research than his peers do.

In section 2, I focus on one aspect of this dual institutionalization … namely, the research specialty and its supporting peer-group. In particular, I analyse the incentives and opportunities in play in "urban" or "polycentric professional" research specialties, concluding, as foreshadowed, that any given scholar is likely to focus her activities on well-

worn research paths – such specialties as are the realm of Kuhn's (1970) "normal science" – and, since the incentives for one are common to all, that the specialist peer-group is susceptible, as Chubin and Connolly pointed out (1982, pp. 294, 299), to an "unproductive over-concentration on some few problems" and hence, potentially, to "undue persistence in worked-out, low-yielding research" approaches by the relevant peer-group as a whole. This is the problematic phenomenon which it is my main business in this paper to understand.

In section 3, I focus on the second aspect of this dual institutionalization … namely, the department and its collegium. In particular, I consider how, notwithstanding the potential for a peer-group to become trapped by a dominant research approach, each individual peer also belongs to a departmental collegium whose members can give the scholar access to forms of comment on or criticism of her specialist research work that she cannot expect to get as reliably from within the peer-group. Indeed, the collegium, assembled, perhaps, for a departmental work-in-progress seminar, can broker forms of knowledge and, indeed, forms of comment and criticism that create the potential for a "wider dispersion of efforts" within the research specialty. As Ronald Burt charmingly put it (2004, p. 388): "People connected to groups beyond their own can expect to find themselves delivering valuable ideas, seeming to be gifted with creativity. This is not creativity born of genius; it is creativity as an import-export business." And it is this business that may well be transacted in the work-in-progress seminar where, indeed, each participant *is* connected to groups (i.e. their various research specialist peers) beyond the one embodied in the seminar itself. Each participant potentially brings something from her peer-group to the assembled departmental colleagues that is not already common knowledge within the collegium and that therefore may be of value to the presenter in his role as specialist researcher.

On this account, then, the successful pursuit of growth of knowledge depends, in "urban" disciplines, on how scholars balance incentives in play within a research field overseen by their specialist peer-groups on the one hand and, on the other hand, opportunities afforded by their membership of departmental collegia. The dual institutionalization of the disciplines is thus offered, in effect, as an antidote to threats to the growth of knowledge that are associated with "unproductive overconcentration" on "worked-out" lines of research activity. The analysis as a whole thus provides another interpretation of Kuhn's resonant phrase "the essential tension". On this account, that tension is between colleagues and peers and its management is important for "growth of knowledge".

1. The dual institutionalization of the disciplines

Following crucial earlier contributions (especially Becher, 1989; Toulmin, 1972; Whitley, 1984), there is a growing literature on the disciplines (e.g. Abbott, 2001; Hyland, 2012; Jacobs, 2013; Krishnan, 2010; Lewis, 2013; Post, 2009; Trowler, Saunders, & Bamber, 2012). Notwithstanding considerable diversity in their approach to the topic, most commentators agree about one thing: *any given discipline has a dual aspect.* (See for example Abbott, 2001, p. 126; Hamlyn, 1992, p. 128; Jacobs, 2013, pp. 28, 29; Toulmin, 1972, pp. 142, 154; Whitley, 1984, p. 57.)

For current purposes, the most perspicuous representation of the duality of a particular discipline is one that points, on the one side, at the *departments* through which that discipline is embodied at various universities and, on the other side, the numerous research *specialties* which disciplinary practitioners have developed. Associated with the department is the collegium, as I will call it, or the group of colleagues who jointly constitute the membership of that organizational unit. Associated with any given specialty is the peer-group, as I will call it, or the group of individuals who are recognized by each other as working on the same

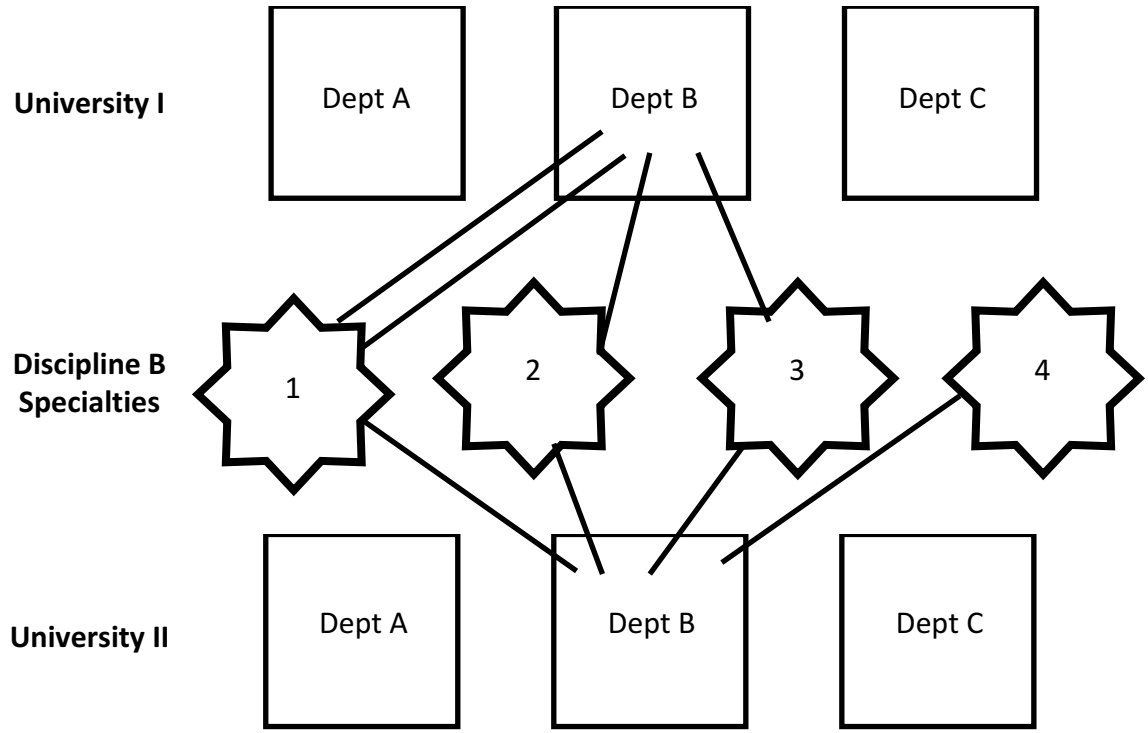
scholarly problem or topic, as we can see through patterns of citation, choices of referees, attendance at specialist symposia, and the like.

This particular formation is, of course, an historically contingent one. Indeed, disciplines themselves, in the modern sense anyway, are largely nineteenth or in some cases twentieth century institutional forms. The particular "lamination" of a department of colleagues consisting of representatives of a variety of specialist peer-groups arises, historically, with the formation of the modern research university and with the understanding, promoted by Wilhelm von Humboldt in relation to the University of Berlin, that teaching, a collective responsibility of departmental colleagues, is inextricably linked to research, carried out on any particular site by those same individuals, but in this case in relation, for each of them separately, to a dispersed peer-group of specialists investigating the same phenomena. Already anticipating some of the key points of the present analysis, Humboldt could, in 1809 (translated by and quoted in Wellmon, 2015, p. 225), say that:

> The teacher isn't present for the sake of the student alone. Rather, students and teachers come together for the sake of knowledge. … Indeed, the pursuit of the teacher's own goal, knowledge, is best achieved through a linking of his strengths with those of his students. The teacher is more practiced, but for that reason, he also tends to be narrower in his views, and to have a less lively imagination. The students are less experienced, to be sure; but with greater openness, they strike out intrepidly in all directions.

In any event, whatever the appropriate "origin story", once the teaching-research nexus was established, and eventually propagated via North American initiatives at Johns Hopkins and Cornell universities (see for example Gerber, 2014, pp. 32-41), this laminated institutional formation was sustained on account of its functional stability. The logic is roughly as follows. Undergraduate teaching by colleagues institutionally sustains the department, for example via income from student tuition fees, in return for which students have opportunities to acquire skills and knowledge presented as valuable to their own personal and professional development that are, to close the loop, the products of the collective research activities of the peers who represent on that site the various research specialties on which the discipline supervenes. (See for instance Abbott, 2001, p. 126; Jacobs, 2013, p. 30.) If the genesis of dual institutionalization lies, as suggested, with the "Humboldt model" of the relationship between research and teaching, then the continued viability of this very model depends, in turn, on the reciprocal relationship between teaching, as a source of funding, and research as involving a set of skills that can be transmitted by teaching and that are recognized by students as a valuable personal acquisition.

Diagrammatically, we therefore have something like this arrangement of departments and specialties, taking into account the point (note 5) that each department exists in a two-dimensional space, with other departments of the same kind at other universities on the one hand and, on the other hand, other departments of different kinds at the same university.



In the diagram, two staff at University I are engaged with the #1 specialty in discipline B, whereas no member of this department is engaged with the #4 specialty in that discipline. Members of the University I department have colleagues (fellow department members) who are engaged in three different disciplinary research specializations and three members of the University I department have a peer (at University II) working on the same specialty as they are.

These two institutional aspects of the discipline – department and specialty – operate, notwithstanding their lamination, on different principles.

The Department

While there will of course be differences among the various manifestations of the discipline, the department is typically charged, *inter alia,* with the

- provision of undergraduate education in the discipline, through a suite of courses constituting a major (field of study) and covering the main aspects of the discipline; and hence
- development of a curriculum and of pedagogy and assessment practices for the courses constituting the major (and including also such "service" teaching as the department might do for other programs at the university – e.g. Business Ethics, Statistics for Psychology, Chemistry for Engineering);
- recruitment and training, in the case of PhD-granting institutions, of postgraduate students at least some of whom will, in due course, be placed into tenure-track positions in university departments where they will, in effect, contribute to the reproduction of the discipline at those sites;
- recruitment, professional development and tenuring of academic staff;

- management of adjunct or contract teaching staff and of other support staff;
- management of the department's physical plant, its resources, including its budget, and the like.

A department is constituted, essentially, by a group of colleagues, or collegium, whose members have joint responsibility for the various academic, administrative, management, and strategic aspects of the instantiation, at that location, of the discipline that they represent there.

The word "collegium" is a term of art that I am using to characterize the members of a department that represents a particular discipline at a specific university.[6] One of the key organizing principles for the department (as one element of the dual institutionalization of the discipline) is that it has to make adequate provision, in its staffing arrangements, in its collegium in other words, for expertise in each of the various subdisciplines or topical areas on the basis of which the discipline is arranged for teaching purposes. Typically, each member of the collegium will have been hired on the "one of each" principle, as D.W. Hamlyn put it (1992, p. 131), meaning, in particular, that each member of the collegium must, separately, command at least one of the subdisciplines or topics that the discipline supervenes on and, collectively, that the collegium as a whole must, ideally, command all the relevant subdisciplines or topics. It is only on this basis that the department can offer a major, taught by experts in the various subjects, that fully represents the discipline.[7]

The collegium in any department of even moderate size and quality will therefore *represent* the discipline in its key constituent areas and special fields. The department is, in short, a microcosm of the discipline in this respect. This makes it, if you like, a "visible college", on the model of Diana Crane's influential idea of the "invisible college" (Crane, 1972, p. 53) which consists of influential research leaders, drawn from different research domains, who transfer information from their own domains to other members of the "college". In principle, and bearing in mind that departmental colleagues may not all be "research *leaders*", they are nevertheless in a position to transfer useful information from one subdisciplinary or topical setting to another, a matter of some importance, to which I return in section 3.

The Specialty

One of the most distinctive features of the (contemporary and recent) disciplinary scene is the degree to which research activities are often highly specialized, so that, say within a given department, even a relatively large one, what one scholar is working on will overlap, if at all, with that of other scholars only at quite a high level of abstraction. As Crane already put it (1972, p. 12), scholarship "as a whole appears to consist of hundreds of research areas that are constantly being formed and progressing through [various] stages before tapering off." Whitley (1984, p. 295) identified a key driver.

> Specialization has been encouraged by the large [post-War] increase in numbers of scientists competing for reputations … Thus scientists narrowed their topics and foci to avoid direct competition but have been able to claim contributions to intellectual goals …

At a certain level of abstraction, all collegia are the same, whatever the discipline, because or at least to the degree that they all have the same basic responsibilities for teaching and management. Notwithstanding some commonalities, peer-groups, on the other hand, will differ from one another, given the different ways in which the specialized scholarly research endeavor is organized in the various disciplines. Crudely, and using Becher's typology (1989, ch. 5), there are "rural" and "urban" forms of research specialization. (Whitley's analysis

(1984) is more complex and systematic, but, for my purposes, the urban/rural distinction is adequate.) As Becher put it (1989, p. 79):

> Urban and rural specialisms differ in a number of ways … Urban researchers characteristically select a narrow area of study, containing discrete and separable problems, where their rural counterparts typically cover a broader stretch of intellectual territory in which the problems are not sharply demarcated or delineated. The urban population is clustered around comparatively few salient topics, as against being spread out thinly across a wide range of themes. The urban mode is to tackle questions with relatively quick, short-range solutions, whereas rural researchers are liable to become engaged on long-range issues which may take years to puzzle out.

For present purposes, I confine my attention to the urban disciplines, noting that this category encompasses a great many of the "hard" sciences and, latterly anyway, the discipline of philosophy, at least in the Anglosphere mainstream.[8]

I note, in particular, three features of urban research specialties.

First of all, in this kind of disciplinary research specialization, the peer-group, as Whitley put it (1984, p. 69), sets priorities and provides a general orientation to the researchable fields that it commands. Or, as Torgny Roxå and Katarina Mårtensson say (2009, p. 213), "academics tend to rely on … [a] rather large [network] involving sometimes hundreds of individuals, [which is] drawn upon for referencing and provision of guidance or orientation". More specifically, it will be the peer-group that, as Chubin and Connolly put it (1982, p. 293), identifies "some finite set of research topics which are seen as legitimate, interesting, and feasible by the members of the specialty." (This is one way in which the "urban" disciplines differ from the "rural" disciplines … in the "rural" disciplines agenda-setting is not this restrictive; individual scholars are much freer to pursue research across a wide range of topics.)

Secondly, it is the peer-group that defines and enforces the standards of evaluation against which any given scholar's contributions to the development of the specialist research agenda are measured. As Stephen Turner says (2000, p. 52), "[t]he fact that a lot of people are trained in fundamentally the same way makes it possible for them to effectively make judgments about the quality of the work done by other people …". Indeed, such a shared background might be necessary for such judgment. Certainly, Michael Mulkay thought so (1977): "[T]rustworthy assessments of the quality of a given piece of work can *only* be made by those who are working on the *same or similar* problems and who are known to be capable of producing results of at least the same level of quality" (quoted from Becher, 1989, p. 61, emphasis added). (Again, there are differences in this regard between the "urban" and the "rural" disciplines. In a "rural" setting, there may well be a range of different *standards* of evaluation as well as researchable topics, associated, perhaps, with different "schools" within the discipline, as, for example, with "close reading" versus "historicist" approaches to literary studies.)

Thirdly, to the extent that the members of the peer-group, superintending a particular specialty, share priorities and standards in this way, they will, as Ken Hyland puts it (2012, p. 205), draw on these shared resources as "important ways of signaling [to each other] a competent disciplinary identity". (Cp. Sullivan, 1996, p. 231.) The peer-group will, then, be an important constituent, notwithstanding its geographical dispersion (especially as compared with the collegium), in an individual scholar's professional persona.

All this adds up to the fact that members of a particular specialist peer-group will read and expect to be read by individuals whom they meet from time to time at on- and off-site

symposia organized around the timely or significant issues within the specialty. Many of these interactions, real or virtual, will leave traces, in particular, in their citational practices as authors of research monographs or articles. As Diana Crane already noted (1972, p. 12), "the literature of basic science consists of tightly knit clusters of papers [citing each other]… [which] represent research areas, sets of closely related problems that … are viewed by the scientists who study them as discrete entities."

If individuals are, *inter alia,* "trained in the same way" and share a common understanding of which research topics are important and, indeed, of how to judge success in problem-solving, then, when an individual scholar interacts with the members of this sort of "social circle", she encounters individuals who are, in relevant respects, a lot like her. She inhabits, in Burt's terminology (1992, pp. 47, 17), a *dense network* where "[i]nformation circulates at high velocity" and "each person knows what the other people know". Such common knowledge is, of course, an important resource, and permits the sort of persistent collective enquiry that will, even in the face of competition for recognition and esteem, create the conditions for normal scientific growth.

Notwithstanding these crucial affordances of competent research practice, the facts that "a lot of people are trained in fundamentally the same way" and use the same standards to judge each other's work and, crucially, work, typically, in narrower and narrower ranges have important implications for the phenomena, identified by Chubin and Connolly, of "undue persistence" and hence will be considered, in relation to the "dynamics of research" in the next section, where I will focus, *pro tem,* on one aspect of the dual institutionalization of disciplines, namely the research specialization and the peer-group whose activities embody it. Of course, any given peer is also a colleague, and I will consider the possible influences of his colleagues on his activities as a peer in section 3, thus acknowledging the other crucial aspect of dual institutionalization. Focusing initially on the specialization and its supporting peer-group will nevertheless bring us, I think, to a more complete understanding of what Humboldt had already noted about the specialist peer – namely, that "he … tends to be narrower in his views, and to have a less lively imagination." This is, of course, precisely the issue that motivates this analysis.

2. Dynamics of Research in an Urban Disciplinary Specialty

Work on urban research specializations shows some important "fine structure". So, crucially, we have the ideas of blockbuster, research trail, and research front, which we can understand, crudely anyway, as follows.

As Chubin and Connolly put it (1982, p. 295), a specialty "can be conceptualized as the confluence of several research trails, each representing a sequence of work by an individual or a small team of researchers … [and] distinguished by some continuity of focus – be it methodological-theoretical and/or problem-oriented – in published research". (Hyland, 2012, p. 32 uses the term "topic" in roughly the same sense.) As Chubin and Connolly (1982, p. 301) point out, within a given research speciality,

> the individual researcher and those powerful gatekeepers of the relevant hierarchies who control funding, publication, and acclaim acquire a reputational interest in the continuance of the trail. Indeed, accumulation of such interests lends legitimacy to the trail and advantages to prominent researchers within it … .

If a research *trail* represents a diachronic feature of specialized research activity, the research *front* can be taken to represent the leading-edge of that trail. As Phineas Upham and Henry Small put it (2010, p. 16), repeating some points made by Crane:

> New scientific findings may initiate the process of front formation by attracting the interest of more scientists who form social ties and generate more findings. The relevance and bearing of each new finding is continuously defined and evolved by the group. … This combined intellectual and social process is seen most vividly in the publications and citation patterns … It is manifest in the emergence of clusters of highly cited papers representing the key scientific findings that are cited jointly.

Indeed, the characterization of a research front given here is highly resonant with a more florid description by Keith Brouthers *et al.* (Brouthers, Mudambi, & Reeb, 2012, p. 959) who argue for a proposition that they label "the blockbuster hypothesis", namely, that "the creation of new knowledge is dominated by a few key insights that challenge the way that people think about an idea" and that these sorts of insights "generat[e] high interest and use", especially when compared with the "numerous studies … [which] have little impact on the frontiers of knowledge" and hence attract very little attention and peer-group recognition.

On this account, then, specialist research in urban disciplines is typically characterized by a blockbuster contribution, initiating a research trail, which develops into a research front which new entrants into the peer-group will encounter as a given and to which, for various reasons (to be considered shortly), they will orient in planning and executing their own research activities.[9]

From the point of view of both new entrants and established scholars, there are, then, a number of factors which secure adherence to the priorities and standards definitive of the specialty. As Chubin and Connolly (1982, p. 293) put it, "individual researchers attempt to choose their research topics 'sensibly' – neither randomly nor omnisciently – in pursuit of bounded self-interest". Noting that the parameters of "sensible" decision-making about topic-selection and other research decisions are subject to considerable uncertainty, they say (1982, p. 294):

> The researcher must decide to pursue a trail with considerable uncertainty as to its final yield; and the uncertainty is greater for studies further down the trail than for the next one or two. It is this rising uncertainty that [means that] … the researcher's planning horizon is necessarily quite restricted.

In this context, there are a number of reasons why the individual scholar working in an urban specialty will characteristically choose to work on well-established research trails, rather than attempting to blaze a new one of her own. Taken together, these reasons constitute an explanation, I believe, for something that Kuhn alluded to … namely, the *formation* of a peer-group pursuing normal scientific work. They also explain the "dynamics of research" that sometimes foster "unproductive overconcentration" on "worked-out" lines of research activity.[10]

First of all, writing about a topic already agreed, by the peer-group, to be legitimate and important *reduces risk,* compared with other options. (See for instance, Foster, Rzhetsky, & Evans, 2015, p. 877 for an analysis based on bibliometric examination of publication patterns in urban disciplines.) As Whitley put it (1984, p. 27), "few are going to risk their futures by trying to publish material which deviates widely from current orthodoxies", including orthodoxies, established via the normal-scientific canonization of blockbuster research, about what it's important to be researching. There are a variety of contributing factors here, including, especially that:

- many early career scholars are vulnerable, in terms of their career trajectory (Abbott, 1999, pp. 140, 169): "the modal would-be author was now a young person whose career was at risk";

- the successful exploitation of existing templates is more tightly connected to rewards, or more generally, to feedback than is the development of new ideas and approaches (March, 1991, p. 73);
- a *recognizably* competent contribution is more easily made by running variations, in an unexploited area of the current research front, than it is by taking a wider or deeper exploratory approach which may be difficult to evaluate (Whitley, 1984, p. 101),
- a well-developed and widely-used procedure or approach is easier to use and more reliably produces results of some kind or another (Levitt & March, 1988, p. 322);
- the activities of scholars working on a mature research trail "cumulatively monopoliz[e] attention at the same time that attention is drained away from alternative modes" (Collins, 1998, p. 15); and
- contributions will be judged for their worthiness, especially in relation to publication and citation, by members of the research specialty peer-group, who face the same choices (Whitley, 1984, pp. 87-88).

Anyone who has acted as a referee or assessor will understand these points. Because or to the degree to which there is already widespread agreement about what topics are significant and what counts as making a contribution with respect to one of these topics, it is easier to assess work that is oriented to one of these significant topics and that shows the markers of being a contribution to that topic than it will be to assess work that, as it were (Collins, 1998, p. 15), "changes the subject" (or tries to) by introducing new and unfamiliar topics or by approaching even familiar topics in an unfamiliar way.[11] Indeed, recent bibliometric studies seem to confirm this "bias" in favor of relatively restrained or conservative contributions to the literature. As Jian Wang *et al.* note (2017, pp. 1422, 1425): "novel papers encounter obstacles in being accepted by journals" because "evaluators give lower scores to papers that are highly novel". (Cp. Boudreau, Guinan, Lakhani, & Reidl, 2012, pp. 24-25.) With standard-issue topics and criteria of significance, judgment is easier and, across different individuals, more reliable than it is otherwise. Furthermore, if a scholar attends a conference and presents a paper making a "conservative contribution to the literature", there are sure to be members of the audience equipped to comment with some competence on his work … something that couldn't as reliably be counted on with other choices of topic.

Secondly, working on an established research trail doesn't have the same "set-up costs" (Chubin & Connolly, 1982, p. 297) as would be associated with working on some other topic. Certainly, by the time a normal science trail has been established in a particular research area, scholars will have had to master the apparatus associated with that trail as part of their own postgraduate education or postdoctoral training and may well have had to teach the topic(s) for undergraduate students. They have already invested in mastering the apparatus, in other words, an investment on which they can continue to draw without significant additional effort. To tackle some other problem will typically involve higher costs in this sense.

Thirdly, writing about an established topic using widely-accepted tools gives a scholar an opportunity to exploit "positive network externalities" (Teece, 1998, p. 523). So, in particular, because the tools and concepts they use are the same as those being used by others, what each does will complement what the other does, so that they can draw on the work of others in a way they couldn't if they were, by implication, attempting to forge a new research trail of their own. Such a "well-trodden path" represents, in effect, a materialization, in Steve Woolgar's terminology (1995, p. 163), of earlier research activities by others, so that the individual scholar setting out on such a research trail unavoidably encounters "off-the-shelf" problems and tools which, as Philip Kitcher put it (2001, p. 112), "stabilize and solidify [and] … set the scene for the next set" of activities. They have a prefabricated tool-

kit of concepts, problems, rhetoric, and techniques. They don't themselves have to develop these tools *de novo* and, importantly, they don't, if they employ these prefabricated elements, have to establish the legitimacy of their deployment. Accordingly, such an approach "decrease[s] the cost of learning" (Zhou, 2002, p. 262) and delivers "increasing returns" to participants (Arthur, 1994), making it, other things being equal, sensible to adopt such an approach.

The individual scholar, on this account, is engaged, in the urban disciplines, in developing a research trail and there are, intrinsic to this situation, some potentially conservative pressures. In order for her work to be recognizable as making a contribution, she has to consider its topicality (is her project on the peer-group's research agenda?) and its "soundness" (will she be seen by her peers as displaying competence in the use of the peer-group's tool-kit?). These considerations may, and typically will, be enough to steer her choices in the same general direction as others, facing the same choice-situation as she does, thus creating the "overconcentration" that Chubin and Connolly have drawn attention to. There will therefore be, within the peer-group, a great deal of normal science "theory churning", as Abbott called it (1999, p. 177), in which individuals run variations on a theme,[12] thus avoiding the risk of being misunderstood or taken for incompetent, but only by risking, collectively, real scientific growth.[13] As Chubin and Connolly put it (1982, p. 294):

> [T]here exist important pressures which lead to undue persistence of individuals in some research trails rather than others; … the social processes associated with the development of these trails tend toward conservative pressures for intellectual continuity on new entrants; and … the aggregate result of these processes is that far from a wide dispersion of research efforts around the boundary problems of a specialty, there will be unproductive over-concentration on some few problems, while high-potential areas go underdeveloped.

This sort of development is what Levitt and March (1988, p. 322) call a "competency trap", by which they mean, in particular, that scholars can, individually and collectively, default to the examination of approved topics using familiar tools because, to put it crudely, it is easier and less risky to do that than it is to pursue "the road not taken". But all these easy choices, each sensible from the point of an individual scholar's cost-benefit ratios (Bourdieu, 1975, p. 33), mean that there is considerable collective risk that the peer-group will, in the language of Chubin and Connolly (1982, p. 299), "persist in [a particular approach] well beyond the point at which additional studies can be justified by their scientific yield". As Jerry Jacobs put it (2013, p. 14), "[s]o powerful are these inward-directed forces that topics at the margins or peripheries of fields lie fallow and are neglected." This is a threat to "growth of knowledge" that, as already indicated, is not reducible to or even very closely connected with anomaly or heuristic degeneracy. It is a threat that arises, if you will, not at the level of relations between statements (e.g. lack of evidential support for theories), but, rather, at the level of community demography – how diversely do individual members of a specialist peer-group enact their scholarly commitments?

3. The affordances of the department

As Chubin and Connolly put it (1982, p. 306), "[t]he challenge to science policy-makers is to find ways to offset, rather than exaggerate, the destructive feedback loops" that, on my account, generate conservative forms of activity (e.g. Whitley's "minor variations" or Abbott's "theory churning", leading to "undue persistence").

My hypothesis is that one way to offset such feedback loops is offered by the kinds of commentary on her work that the scholar receives (or can receive) from her departmental

colleagues. My key point is that her colleagues differ in various crucial ways from her peer-group, and that these differences may promote a loosening of the restrictions that the peer-group might encounter and reinforce because or insofar as they give the scholar access to forms of comment on or criticism of her work that she cannot expect to get as reliably from within the peer-group.

The social psychology of departmental collegiality

To be concrete and specific about this potentiality, I will embody in the departmental *work-in-progress seminar* a number of features that will, of course, be present elsewhere as well in the disciplinary ecosystem. The seminar is, if you will, a metonym for what is certainly a diverse set of opportunities. This is not an innocent choice. Certainly, as Wellmon for example has noticed (2015, p. 235), it is the device of the seminar, developed at the University of Berlin in the early nineteenth century, that creates, historically, the conditions for the inculcation and application of the scholarly virtues: "It was the seminar that first disciplined the disciplines and institutionalized the logic of science … ."

To avoid confusion, the seminar is an event or occasion in the working lives of the members of the collegium. There are, of course, other such events or occasions, such as departmental meetings, where administrative matters or hiring might be discussed.

I consider five points about this sort of occasion.[14]

First of all, because the peer-group and the collegium are organized on fundamentally different bases, when a scholar presents a departmental work-in-progress seminar, she will be in the presence of colleagues who, individually, command the various subdisciplines or topics and, collectively, command the discipline as a whole, at least at a certain level of abstraction. (This will, typically, not be the case with her peer-group, and, hence, with her conference audiences, the referees of her journal submissions, and the like.) More specifically, because or to the extent that there are, within *other* subdisciplines or research specialties, concepts, theories, or techniques that might be relevant to a scholar's problem, these will, *ceteris paribus,* be more likely to surface in a departmental seminar than in a conference presentation, if only because these other areas of enquiry are represented by expert practitioners in the seminar audience as they might not be among the scholar's peers. This means, in Burt's terms (1992, p. 23), that the collegium is a "sparse network" of individuals – in contrast to the "dense network" constituted by the peer-group – because its members (who are, of course, each also members of one or more "dense networks" through their research affiliations) are not "redundant" – they don't all know the same things – and hence membership of a collegium of this kind "ensure[s] exposure to diverse sources of information".

Secondly, because most members of the departmental collegium will not command the subdiscipline or topic and, *a fortiori,* the research trail to which the seminar paper is meant to be a contribution, their understanding of the issues, if any, is likely to be dated and partial, depending on their exposure, perhaps years before during their education and training, to then-current literature in the general area in which the work reported has been conducted. Departmental colleagues will therefore need, in order to follow the paper, to ask questions of the scholar that a fully paid-up specialty peer would not, *ceteris paribus.* (For there is, on any particular research trail, a lot of common knowledge and taken-for-granted assumptions, which don't need to be thematized during discussion within the peer-group, that facilitate easier access to the argumentation of the paper.) The collegium will be able, in other words (Payne, Bettman, & Schkade, 1999, p. 246), to subject the research paper to a "sensitivity analysis", which questions the dependence of the *results* of the argumentation in the paper on

assumptions made by the scholar, in some cases at least on the basis of a shared approach to this form of research. Certainly, one of the things we know, both from our own experiences as teachers, but also from the social psychology of knowledge-formation, is that it can, and often does, happen, as a result of such questioning, naïve from the point of view of the research trail, that the scholar is provoked to think more deeply than she has to when in the presence (virtual or actual) of others who share her assumptions. This is known, in the psychology literature, as "expectancy disconfirmation", which, according to Norbert Kerr *et al.* (1996, p. 696, emphasis added), "has been shown to undermine judgmental confidence and *promote more systematic processing of information*". A scholar's colleagues' naïve questions and comments in other words provoke her to double-check what she has taken for granted within a specialist framework of discussion and, at least sometimes, to begin to consider, or perhaps even to discover, that what has been taken for granted ought, at least, to be questioned and perhaps even to be abandoned. This is a kind of learning that is, *ceteris paribus,* less likely to occur within the specialist peer-group, where a shared set of assumptions makes expectancy disconfirmations less likely to occur.

Thirdly, because, or to the extent that, departmental colleagues are (relatively) unengaged, perhaps even disengaged, from the scholar's research trail, they will not be *committed*, as a matter of their individual scholarly identities, to any of the positions which peers working on that trail have articulated. Accordingly, if the scholar offers, as part of her presentation, an idea, an argument or a technique, that has a certain discursive status within the research trail peer-group, it will not automatically be marked, as it would be for the peer-group, as having that status for the departmental collegium, whose members can therefore assess it, and indeed discuss it, in perhaps quite a different way than it would be assessed or discussed by the specialty peer-group. Suppose, for instance, that the scholar offers an observation that would be controversial within the specialty peer-group, perhaps by departing in some ways from the current paradigmatic "template" for recognizably competent research, and thus perhaps opening up a point of contact with a "high-potential area … [which might otherwise] go underdeveloped". Insofar as her observation is not marked as controversial in the seminar presentation, local colleagues encounter it without that understanding of its significance within the specialty. They will neither be bothered nor excited by the "controversial" aspects of the presentation and hence will not be focused, as a peer-group almost certainly would be, on these particular aspects. And this means, *ceteris paribus,* that all the various aspects of the scholar's presentation are more likely to receive attention in the local, departmental seminar before colleagues than they would when the paper is presented, say at a conference or symposium, to a predominantly specialist audience, who will tend to focus on its "controversial" aspects, and, indeed, perhaps react rather repressively or slightingly to them.

Fourthly, because the work-in-progress seminar is, in principle, an occasion for collegiality among scholars who are also mutually engaged in teaching and the administration of a local organizational unit (the department), because they have, at least in these matters, a shared responsibility to and for each other, the seminar paper will be heard in a different modality than the article submission will, at a later stage, be read by referees and editors. In particular, while there are sometimes, of course, difficult colleagues, who are anxious to score points at the expense of the scholar presenting his work, the default assumption at the work-in-progress seminar is, typically, that this is a chance to contribute to the improvement of that work. (This is not the starting-point for most referees, who see their duty as involving triage and, at low-acceptance journals, as oriented primarily to rejection as the default judgment.) After all, the reputation of the department depends, in some measure, on the recognition that its various members receive for their contributions to the research trails with which they are engaged. (Cp. Brennan & Pettit, 2004, p. 209.) If there is something that colleagues,

individually and collectively, can do to improve the scholar's work, with all that that means for its visibility and impact in (and, ideally, beyond) the specialist literature, then that is to their advantage as well. This doesn't, of course, mean that they won't be critical, but it does mean that their criticisms are presented in relation to quite a different assumption (than the one which governs the journal referee). In particular, department colleagues provide criticism relative to the assumption that it will, in its substance, and in its mode of presentation, aid the scholar in improving his work, and this may include, by the reasoning already considered, judging the paper by different standards than are likely to be employed by the peer-group … standards that may well offer the scholar an opportunity to consider less routine ways of contributing to the development of his specialist research trail. So, for example, the colleague will be interested, as the peer might not be, in how well the arguments and claims of the paper cohere with what is known in other subdisciplines or research specialties … and this may well contribute not only to the degree of innovation that the paper is able to display, but also to the likelihood that the paper will be read not just by the peer-group it was intended for, but also by other members of the discipline.

It is important both to frame and not to exaggerate the claims that I have been making in this section. There are two, complementary provisos which it's important to register.

First of all, the individual scholar has to exercise her own judgment about the potential value of the comments her colleagues make about her work. Perhaps the seminar throws up ideas that extend or challenge or simply reframe conventional understandings within the specialist peer-group. The individual scholar still has to decide how to deal with these ideas. Perhaps she sees their potential value but is hesitant to pursue them for any of a variety of reasons (as sketched in section 2 above) … high "set-up costs", the risk of being misunderstood or dismissed by the peer-group. Empirically, different individuals will react differently on these sorts of occasions; some will seize opportunities, while others will not. And of those who seize the opportunities to extend or challenge routine work on the research trail only some will secure peer-group up-take of their ideas and hence drive discussion onto a new trail or take it further than it would otherwise get along the existing trail. The point is not that it's "automatically" the case that work-in-progress discussion is valuable from the point of view of the paper's author or of the research peer-group which she represents. The point is, rather, that the seminar provides a certain kind of opportunity. How any given research trail evolves will depend, in part, on how individual scholars belonging to the peer-group which tramps that trail have reacted to their colleagues and to each other.

Secondly, blockbuster contributions, founding new research trails, are rare. As Brouthers *et al.* put it: (2012, p. 959) "the creation of new knowledge is both difficult and rare … [and] is dominated by a few key insights that challenge the way that people think about an idea". So the work-in-progress seminar is not and indeed cannot be a factory for the routine production of such contributions. That's the matter that it's important not to exaggerate. When the scholar presents her work to the departmental collegium, it is not guaranteed, indeed it is still very unlikely, that what happens there will so enhance her work that it comes, in due course, to have blockbuster status in her research specialty. On the other hand, having said that, something important remains. While it is not guaranteed and indeed remains (highly) unlikely, what the collegium might contribute and sometimes does contribute is more likely to facilitate the significance and originality of the scholar's work than what she will typically get from her peer-group (with their common orientation to the current state of play on their shared research trail). There is no recipe for originality in growth of knowledge, but there is still a difference between being in the kitchen and not being there. The department, not the research specialty, is arguably, and perhaps surprisingly, the kitchen.

All these points, taken together, make it, I believe, more likely that departmental colleagues will assist in the diversification of research efforts than will specialist peers. If the problem of "undue persistence" has a solution, it is on account of the affordances of the disciplinary department. The two aspects of the dual institutionalization of disciplines therefore play complementary roles. The specialization and its associated peer-group function according to a logic driving individual researchers in a common direction (and hence can sometimes produce "undue persistence" or "overconcentration" of research effort). The department and its associated collegium provide, potentially, a counter-weight to this logic, fostering or at least disinhibiting more adventurous contributions by peers to their specializations, and thus offering an alternative to "undue persistence".

The collegium as knowledge brokers

The idea of *brokerage* figures significantly in the literature, across a range of disciplines (e.g. Burt, 1992; Weick, 1976; Wenger, 1998), about innovation as a key aspect of the growth of knowledge. The key idea is that, to innovate, it can be useful and may be necessary to bring together ideas or methods that do not usually sit together, given the existing landscape of research trails and fronts. Someone who has access to more than one research specialty, then, may be able to "broker" relations between or among these specialties by offering an idea or method from one specialty to the peer-group in another specialty where this idea or method can play an important role in the growth of knowledge and the diversification of research activity. As Burt put it (2004, p. 355), "[p]eople familiar with activities in two groups are more capable than people confined within either group to see how a belief or practice in one group could create value in the other." (These are Susan Leigh Star's "wizards" (1995, p. 107), "who know enough about more than one layer to perform rare cross-layering coordination" and, in particular, are able to guide those researchers working in their own specialist fields to work in other fields that might be relevant to achieving new insights.)

We have an illustration of this, I think, in our discussion of the work-in-progress seminar. As indicated, there are specialists in a variety of different fields of enquiry in the room when the scholar presents his work who possess knowledge and skills that may be relevant to what he is discussing which are unknown to him because of the organization of research trails, and, typically, to most of his peers in his own specialist research trail. One of the affordances of the scholar's embedding in the collegium is that it gives him access to standards and ideas that he doesn't (automatically) get as a member of the peer-group. As Helen Longino put it (2002, p. 155) in a different, more general context:

> Membership in multiple communities may be a source of internal conflict, but it is also an epistemological resource. It permits an individual to compare standards and to assess (purported) knowledge produced and accepted in one community in reference to standards proper to another. It confers on individuals the ability to see things from more than one point of view or at least to understand that there could be more than one point of view about a given matter.

Any member of the assembled collegium can be the broker in this situation. The scholar can himself initiate brokerage by "fishing" for expertise from his auditors. Or they can offer that expertise, seeing the possibility that something that they know may be relevant to the problem he is discussing. Of course, this requires mutuality and that has to be earned and managed. The scholar needs, for instance, to give more attention, than he would with a peer-group audience, to the "set-up" of his problem, so that non-specialists, not sharing the common assumptions of the peer-group, can understand what's at issue and can see why it might be important. And his interlocutors have to be willing and able to stretch themselves, to compute analogies while he is speaking, so that they can intervene with suggestions, or even

just queries, about the possible relevance of something they know from their own experience as specialists in other areas. This mutuality is likely to available, however: the reputation of each individual is enhanced by the standing of every other individual in his department, so helping the author improve his work can lead, admittedly by long and tortuous processes, to better opportunities for others as well. As Etienne Wenger put it (1998, p. 109):

> The job of brokering is complex. In involves processes of translation, coordination, and alignment between perspectives. It requires enough legitimacy to influence the development of a practice, mobilize attention, and address conflicting interests. It also requires the ability to link practices by facilitating transactions between them, and to cause learning by introducing into a practice element of another.

Potentially, the stakes are very high in this sort of situation and for two reasons.

First of all, this sort of brokerage can facilitate precisely the sorts of break-throughs that constitute or are at least candidates for being "blockbuster" contributions. As Crane says (1972, p. 104), "new fields are created in part by the discovery of a linkage between old fields", a discovery that might well be facilitated, on my account, by the encounter between colleagues in some suitable departmental setting.

Secondly, the potential for brokerage, via the mechanism of the work-in-progress seminar in our particular case, may, as Karl Weick put it (1976, p. 7), "be an elegant solution to the problem that adaptation can preclude adaptability." This will take some explaining, but, to begin, here's a key worry that sits underneath this whole exposition: There are strong conservative tendencies associated with at least the mature growth of any particular research trail; after a time of initial innovation, a great deal of what happens is relatively routine as the approach taken to the object(s) of enquiry becomes more standardized and better adapted, for instance, to the practicalities of postgraduate training and early-career pursuit of tenured employment. (Cp. Crane, 1972, p. 67.) As Norbert Elias puts it (1982, p. 4):

> As an advance in knowledge … , scientific innovation cannot be easily routinized. If it is, it may get stale, may stagnate, may follow a set track, and be tied to a prescribed method; its representatives may miss unusual openings, and may shun radical innovations.

Or, translating back into Weick's idiom, the increasing adaptation of the tools used on the research trail to the tasks they are deployed to undertake means, more or less unavoidably, that the tool-kit as a whole is less and less flexible in meeting challenges that might lie off the trail or that might block further progress along the trail. Accordingly, those committed to the use of those tools are more and more vulnerable in relation to such challenges. That's what's meant, I think, by the idea that "adaptation can preclude adaptability".

And how does brokerage help with this? By giving the well-adapted (but potentially vulnerable) scholar access to other specialist forms of enquiry, using other tools, deploying other concepts, and engaging with different objects of enquiry. These specialties, commanded by other members of the collegium, constitute "cultural insurance" in Weick's diction (1976, p. 7) because, to be specific, they are only "loosely coupled" to the author's specialty, in the sense that "the identity, uniqueness and separateness of elements is preserved, [so that] the systems can retain a greater number of mutations and novel solutions." If and when her own specialty becomes over-adapted, she still has access to elements that preserve her own adaptability; she gets them, typically, not from her specialist peers, with whom she shares this challenge, but, rather, from her departmental colleagues.

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The growth of knowledge is institutionally provided for, then, through, precisely, the dual institutionalization of the disciplines. Through the peer-group, the individual scholar is provided with an agenda materialized in the persistent collective development of a specific line of enquiry. Through the collegium, the individual scholar is provided with off-agenda ideas, tools, and standards and hence with a source of potentially new contributions to her chosen areas of research. There will, of course, be individual differences among scholars and these too are relevant. Not every scholar will take advantage of the affordances of her departmental collegium … but some will and while only some of them will succeed in "changing the subject" on their research trail, it is the balance between those who hew to the trail and those who establish new trails that we call the social dynamics of the growth of knowledge.

Robert Rosenwein provides a nice overview of some of our themes. In particular, he draws attention to the central "tension" that is at issue within any discipline – between the demands for originality and the affordances of a conservative tradition of enquiry. He says (1994, p. 262):

> [S]cience may be thought of as a social system vehicle for producing knowledge. In this sense, like any other social system, science is characterized by "group processes," for example, recruitment of new members, socialization to group norms, facilitation of group cohesiveness, maintenance of group loyalty, and leadership for continuity and coordination. Further, in this formulation, there is conflict in science, as in any system, between the "conservative" tendency to preserve and protect those modes of thought and behaviour that have been successful and the tendency to tolerate and absorb new ideas, to provide an atmosphere where creativity and criticism can flourish, in short, to provide the possibility of change and growth.

What we might claim to have shown, in our examination of the dynamics of research, is that there are different institutional spheres within which the conservative and creative forces can be given proper expression and that, between them, the balance might well be struck in certain cases. Because the individual scholar is a member both of a departmental collegium and of a specialist peer-group, he is exposed to opportunities and incentives for both conservative and creative responses to his situation. To be sure, he has to balance these in his own way. To be sure, different individuals will balance them in different ways. What is important, I think, is that there is, in effect, provision, within the dual institutionalization of the disciplines, for any given scholar to be *faced* with this problem. This, I believe, is the answer to "[t]he challenge to science policy-makers" which Chubin and Connolly have identified. This is the way "to offset, rather than exaggerate, the destructive feedback loops" that threaten the growth of knowledge.

Here, in conclusion, are the key points:

- The research trail, with its associated peer-group, provides individual scholars with a pre-existing apparatus for the conduct of disciplined research.
- The department, with its associated collegium, is organized differently and, in particular, represents the full range of subdisciplinary expertise.
- In the work-in-progress seminar, this "off-trail" expertise becomes available to a scholar working in a particular research specialty.
- The refreshment of mature and the inspiration for new research trails is therefore going to be provided, *inter alia,* by humble quotidian interactions between departmental colleagues.

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NOTES

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[2] Compare Steve Fuller, who said (1988, p. 3), programmatically, that

> The fundamental question of the field of study I call social epistemology is: *How should the pursuit of knowledge be organized, given that under normal circumstances knowledge is pursued by many human beings, each working on a more or less well-defined body of knowledge and each equipped with roughly the same imperfect cognitive capacities, albeit with varying degrees of access to one another's activities.*

[3] See for example Kitcher (1990). The *locus classicus* is Kuhn (1970, p. 186).

[4] See the Google Scholar citations for Chubin and Connolly (1982) at https://scholar.google.com.au/scholar?cluster=2221477401454438821&hl=en&lr=&as_sdt=2005&sciodt=0,5.

[5] I believe my account is complementary, rather than an alternative, to any internalist account because, of course, anomalies, should they be independently definable, still have to be noticed and acted on. The "dynamics of research" which I try to identify can facilitate or at least disinhibit noticing and acting. My account also provides for off-agenda scholarly activity even in the absence of acknowledged anomalies, and this is a strength, in my view, because or insofar as some "novel" developments in the disciplinary specialties are indeed the result of the recombination of previously isolated ideas, rather than a response to "crisis".

[6] As Daniel Alpert has pointed out (1985, pp. 250-1), any given department itself exists, relationally, in a dual institutionalization, with its "sister" departments in the same university, with which it may be bound in degree programs, funding arrangements, infrastructure support, and the like, and with its cognate departments at other universities, with which it may compete and cooperate in various ways.

[7] Indeed, on some accounts (see, especially, Jacobs, 2013, pp. 27-28), this arrangement (of curriculum mapped onto collegium) is crucial for the viability of a discipline for at least two reasons. First of all, and locally, providing an attractive major for undergraduate students is the budgetary foundation of a department's very existence in most university systems. Secondly, and globally, a discipline will be able reproduce itself only if it can train students according to a curriculum that will enable them, after further postgraduate study and research, to present themselves as fit to teach (elements of) that curriculum elsewhere. In any event, the work of the collegium as such will include (Hyland, 2012, p. 24) "[e]veryday local practices of teaching, supervision, research, marking and committee work"

[8] "Dynamics of research" associated with "scientific growth" in the so-called "rural" disciplines will of course reflect relevant differences. Where there is, as Becher puts it (1989, p. 157), a high problem to person ratio, or, as Whitley would say (1984, p. 88), a relatively low level of strategic or even functional dependence of one scholar on the work of others,

there will, arguably, be less risk of collective persistence in "played-out" research approaches. The risk, in these situations, is, rather, that knowledge will grow, insofar as it does, by an accumulation of unorganized materials rather than through systematization, leading to profounder insight. Accordingly, the role of dual institutionalization in "tuning" the "dynamics of research" will be different.

[9] This, if you will, is one approach to the "fine structure" of Kuhn's "normal science", which he describes (1970, p. 24) as follows: "Closely examined, whether historically or in the contemporary laboratory, that enterprise seems an attempt to force nature into the preformed and relatively inflexible box that the paradigm supplies. No part of the aim of normal science is to call forth new sorts of phenomena … Instead, normal-scientific research is directed to the articulation of those phenomena and theories that the paradigm already supplies."

[10] While there are significant differences between "rural" and "urban" disciplinary research specializations, there are some analogies which render some of the points to be considered applicable to some instances, anyway, of research activity in "rural" specialties. Consider, for example, what might be called "the Foucault phenomenon", where, across a range of humanities and social science disciplines, literary, historical and sociological studies were, from the mid-seventies, increasingly preoccupied with broadly Foucauldian approaches, which became, if you will, more or less canonical, at least in certain circles. (Foucault's work had, at mid July 2019, nearly 930,000 Google Scholar citations; his h-index was 288.) While there was no agreement, even in this situation, about researchable *topics,* and hence no "unproductive overconcentration" on a narrow list of such topics, there was widespread agreement on the approach to be taken to researchable topics and this may well have constituted an instance of "unproductive overconcentration", especially in light of the tenuous grasp, by many participants in this phenomenon, of the actual techniques associated with Foucault's own work.

[11] This particular analysis shows most clearly, I think, how the widely-discussed distinction between contexts of discovery and of justification does not, in any event, cleanly map onto a distinction between empirical (e.g. historical or sociological) analysis, on the one hand, and logical analysis, on the other hand. Referees, readers more generally, are making decisions about how well *justified* an argument or thesis is, and we can understand what they are doing, e.g. in applying epistemologically respectable standards of assessment, in a way that is empirically informed. For acute commentary on the discovery/justification dualism, see Paul Hoyningen-Huene (1987).

[12] Another threat to growth of knowledge sits at a right angle to the unproductive overconcentration that Chubin and Connolly have drawn attention to. This arises even, or perhaps even especially, when individual scholars are running minor variation on approved themes using approved tools, for even this activity requires a degree of "novelty" at least in the sense that the results reported are not already part of the agreed corpus of disciplinary or specialist knowledge. So, in particular, in pursuit of such novelty, within the constraints of conservative normal science, it appears – and the relevant studies are now numerous – that many scholars report results that cannot be replicated or reproduced by others. This is the so-called reproducibility crisis – worthy of examination in its own right. (See, for a good summary and references, The National Academies of Sciences, 2019.)

[13] Geoffrey Brennan and Philip Pettit provide an important analysis (2004, p. 121): "It seems plausible that a system in which enquiry proceeds across a broad front may be superior to one in which all energy is directed at being the first to do what everyone else is trying to do. From the point of view of the system as a whole, it may be better to have scholars choose research agendas that are more speculative, and where the chance of each making a fool of herself (or

simply wasting time, coming up empty-handed) are non-negligible. … In other words, the system as a whole can pool risks that individual scholars may well find daunting. On this basis, there is a problem: individual scholars may choose research agendas that are on average too conservative."

[14] It is important, as a proviso, to understand that I am describing, in what follows, some of the *affordances* of the departmental seminar – some of the opportunities that, potentially, it makes available to the scholar. Whether those opportunities are exploited, however, depends on two other factors: (1) whether the collegium participates effectively, and (2) whether the scholar who is presenting has the will to take advantage of what the collegium offers. These are non-trivial assumptions.